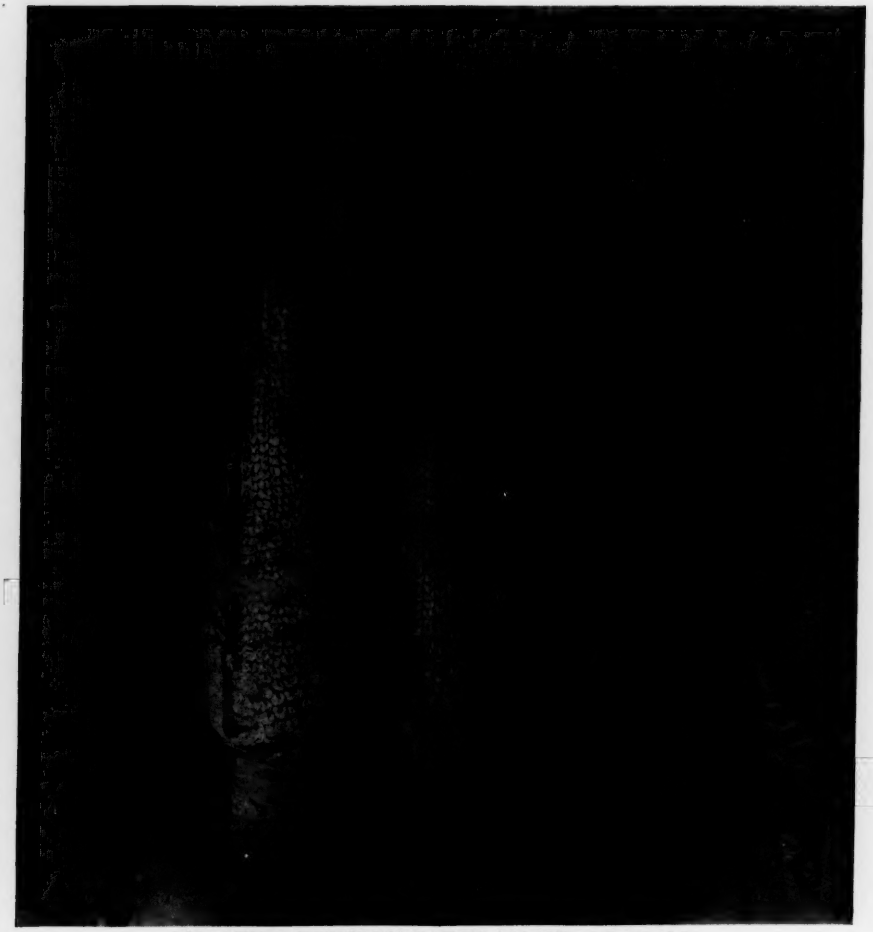
The Journal of HEREDITY



NO SEED WITHOUT THE HAND OF MAN

(See Page 29)

Lysenko's "Genetics"

Human Pedigree Charts

VOLUME 37 JANUARY, 1946 NUMBER 1

The American Genetic Association

An incorporated organization devoted to promoting a knowledge of the laws of heredity and their application to the improvement of plants, animals, and human racial stocks

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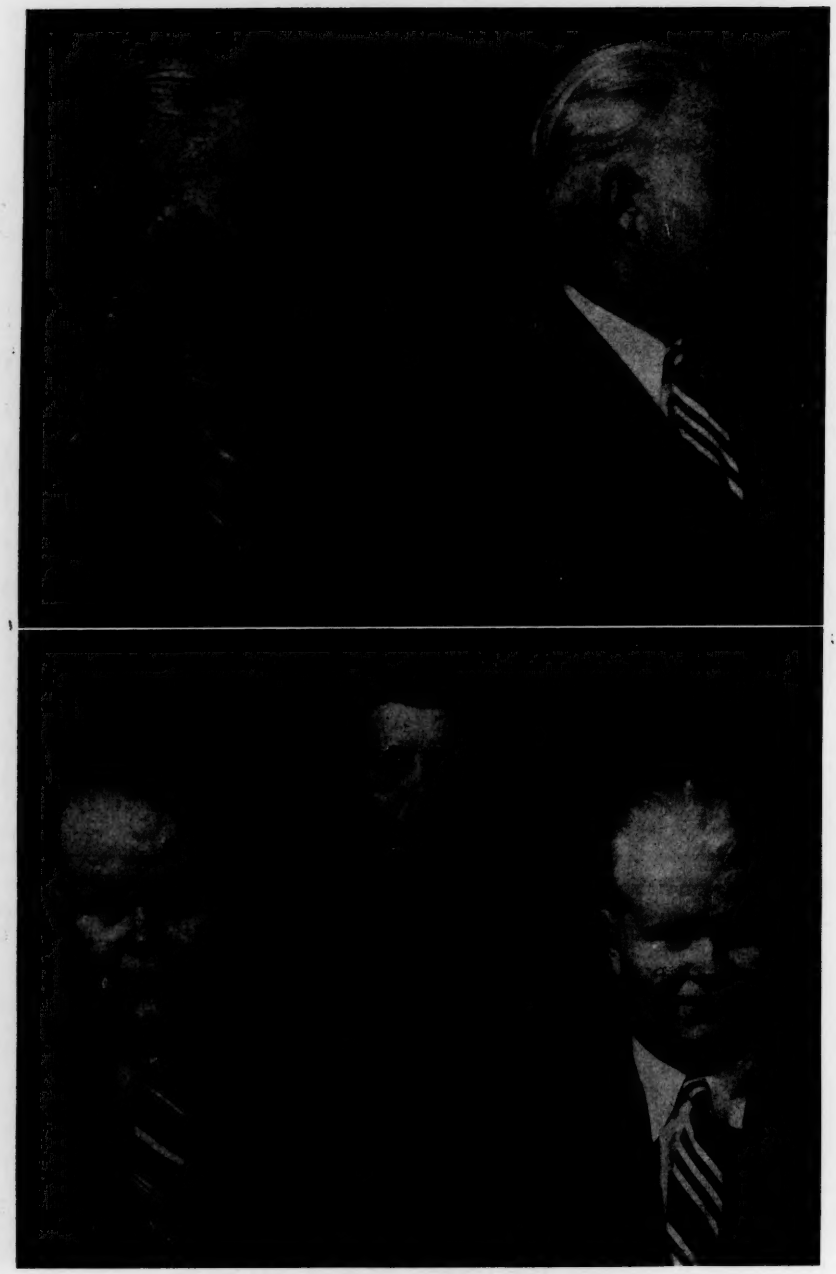

TWO AMONG THREE MILLION

Figure 1

Approximately one person in ten thousand in the United States is an albino; one birth in three hundred is of identical twins. It follows that one birth in three million will be of albino identical twins. Two other brothers are albinos. One brother (see lower photo) and three sisters are normally pigmented.

A PAIR OF ALBINO TWINS

DAVID C. RIFE, M. D. SCHONFELD and MARILYN HUNSTEAD
Genetics Laboratory, Department of Zoology, Ohio State University

ALBINISM occurs in man among all major racial groups. Available evidence indicates that it is due to a recessive allele of the gene conditioning normal pigmentation. Its highest incidence appears to be among the San Blas Indians, where seven out of every thousand persons are reported to be albinos. Its incidence among most populations, however, appears to be not more than one per 10,000.

In the United States about one birth in eighty gives rise to twins. Of these twins about 25 per cent are estimated to be monozygotic, or in round figures, about one birth in 300 gives rise to monozygotic twins. Thus we should expect not more than one birth in 3,000,000 to give rise to albino monozygotic twins.

The albino young men, George and John, shown in Figure 1, are twins. Intra-pair comparisons in respect to a number of traits indicate that they are monozygotic. They have three brothers and three sisters. Two of the brothers are albinos, while the other brother and all of the sisters have normal pigmentation. The parents have normal pigmentation, so both presumably are heterozygous for albinism.

If the twins are monozygotic the eight children developed from seven zygotes, three of which were homozygous for albinism. If the twins were dizygotic it would mean that four out of eight zygotes were homozygous for albinism. In families of eight children, where both parents are heterozygous for albinism, we should expect three out of seven zygotes to be homozygous for albinism over twice as frequently as we should expect four to be homozygous for albinism.

Both members of the pair are of blood group B, blood type MN and are Rh positive. Both are tasters of P.T.C. and neither has hair on the mid-dorsum of the fingers. While no unusual features are manifest in the dermatoglyphics, the prints of all four hands are quite similar. We also obtained the prints of the normally pigmented brother. A comparison of the formulae shows that the prints of the twins are more similar to each other than either is to their brother. The greater similarities of the twins as compared with their brother is especially noteworthy in the total ridge counts.

Note that each of the twins has attached ear lobes. Marked similarities are also apparent in their features. John is the larger of the two. He weighs 238 pounds and is six feet tall. George weighs 230 pounds and is 5'9½" tall. While such an intrapair difference in stature is more than the average in monozygotic twins, it is sometimes found. George and John are strikingly similar in voice, mannerisms and observed personality traits.

Like most albinos they have poor vision and are very sensitive to bright

Palm and Fingertip Formulae

Palms	Fingertips	Total ridge count
George		
Right: 9.7.5".3.-t-V.O.O.O.L.	$L^{u}.L^{r}.L^{u}.L^{u}.L^{u}$.	166
Left: 9.7.5".4.-t-A^{u}.O.O.O.L.	$L^{u}.L^{u}.L^{u}.W.L^{u}$.	
John		
Right: 9.7.5".5'.-t-L^{r}.O.O.O.L.	$L^{u}.L^{u}.L^{u}.L^{u}.L^{u}$.	180
Left: 9.7.5".3-t-A^{u}.O.O.O.L.	$L^{u}.L^{r}.W.W.L^{u}$.	
Brother of the Twins		
Right: 11.x.7.5'.-t-A^{u}/A^{c}.O.O.O.L.	$L^{u}.A^{t}.L^{u}.L^{u}.L^{u}$.	104
Left: 9.7.5".5'.-t-A^{u}.V.O.O.L.	$L^{u}.W.L^{u}.L^{u}.L^{u}$.	

light. They attended a school for the blind, and their vision is too poor to permit them to drive an automobile. Both are bakers and work at the same place. They state that their parents are not related and that no instances of albinism are known among their relatives, outside of the immediate family.

Literature Cited

1. GATES, R. Ruggles Heredity In Man. Pp. 115. Macmillan Co., 1931.

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THIRTEEN YEARS OF PLANT PATENTS

THE plant patent act became law on May 23, 1930. Fifteen months later, August 18, 1931, the first plant patent was issued. Notice was taken in the JOURNAL during the ensuing months of some of the curious paradoxes generated when facts of botany were grafted onto the accumulated legalisms which had encrusted the original Constitutional mandate to "promote science and the useful arts" during a century and a half of patent law applied to mechanical, chemical, and physical phenomena. The plant patent law itself was a curious document. Most of the questions raised at that time have never been completely answered. Court decisions give the only final answer in interpretation of moot points, and no important issues have been tested in court. So the status of plant patents is still mostly indefinite.

Since 1931, 612 plant patents have been issued. Nearly a half of these are for roses (299). Other flowers total 151 patents, and 137 of the patents cover allegedly new forms of fruit or other agricultural products. This represents less than one tenth of one per cent of the patents issued during this period. Plant patents have hardly been big business for anybody.

There have been several suits regarding the patentability or the ownership of patented plants. The owners of plant patent 399 prosecuted an infringement action and obtained title to the allegedly infringed plants by a consent decree.

An interesting summary of the plant patents issued to date, with abstracts of plant patents 85 to 610 has been prepared by Mr. Robert Allyn, 41 Park Row, New York 7, New York. Mr. Allyn, a patent attorney whose father was an amateur plant breeder, is one of the few people connected with the patent industry who has worked for a realistic revision of the plant patent legislation. He has been consistently skeptical of the utility of the plant patent law in the present form. He considers the administration of the law in terms of these questions: "Have farmers, agriculturalists, or plant breeders been benefited? Has the Plant Patent Act promoted progress in science and the useful arts as intended by the Constitution?" The conclusion is not enthusiastic: "The answers to our earlier questions appear to be that Horticulture has received a 'shot in the arm'—if the number of flower patents is a criterion; the wood pulp and match stick growers may profit and a few fruit growers may have profited.

"We have not been able to secure any statistics as to the effect of the Patent Act upon the welfare of farmers.

"Where are the new or improved vegetables?

"Should we not amend the law so as to permit the grant of patents on new plants regardless of the method of reproduction, i.e. those reproducible by seed as well as asexually or should we seek to make the *products* of new varieties 'patentable'?"—R. C.

LYSENKO'S "GENETICS"

A Review

ABOUT a decade ago biologists outside the U. S. S. R. began to hear reverberations of a strange campaign against genetics waged by professor T. D. Lysenko, now member (academician) of the Academy of Sciences of U. S. S. R., director of great research laboratories, and holder of important administrative posts. Since then, the opinions of Lysenko have been mentioned rather often in American newspapers and popular magazines. Some of Lysenko's speeches, together with those of his opponents, have been re-published in English translation. It may seem strange, then, that American geneticists, and with few exceptions those of other lands outside the U. S. S. R., have either ignored the controversy entirely or at any rate refrained from answering Lysenko's challenges.

Two causes explain this forbearance. First, it did not seem altogether fair to criticize the opinions of a man whose main writings are accessible to only a minority of geneticists who are able to read Russian. This cause is now removed by the publication of a translation of Lysenko's *Heredity and its Variability,** in which the author gives a systematic presentation of his views held as late as 1943-1944. The views expressed in this volume thus represent the author's conclusions after he had reached his present highly responsible position.

The second cause has been a reluctance to criticize adversely the scientific output of the U. S. S. R. This consideration weighed especially heavily with those Americans who are friendly to Russian science, and who justly desired to avoid anything that would prove in the least mischievious to the scientific or general relations between the two countries. It is the considered conviction of this reviewer that the above reluctance is misapplied, and particularly so when a scientific theory has strayed so far from the truth as that of Lysenko so obviously has. Nations, like individuals, may justly resent off-hand derogations of their domestic affairs by casual acquaintances. But to withhold well-considered and constructive criticism is unbecoming to scientists, and least of all to friends among scientists. Such an attitude in reality implies a subtle disrespect, not unlike a distainful decision to overlook an obviously bad musician in a mediocre orchestra. Now, science in the U. S. S. R., and particularly the science of genetics, does not merit such treatment.

Lysenko opens the attack on the "Mendelian-Morganian" genetics in the very first paragraph of his book: "In all reference and text books on genetics, heredity is understood as reproduction of like by like. In my opinion, such a definition gives little basis for understanding the phenomenon of heredity." The author is obviously unaware of the fact that in modern genetics heredity is conceived of as transmission of a norm of reaction of the organism to its environment, and not as mere similarity of the externally visible structures of the parents and offspring. Geneticists "were and are using methods and approaches which do not permit them to learn anything about the essence of heredity." These "methods and approaches" are hybridization of different organisms, and they are faulty because hybridization leads to "mixing their breed through crossing." The author should have recalled that the main tenet of genetics is precisely that no mixing of the hereditary elements, genes, takes place in hybrids. To him, heredity is "the property of the living body to require definite conditions for its life, its development and to react definitely to

*Lysenko, T. D. Heredity and its Variability. Translated by Th. Dobzhansky. King's Crown Press, 65 pp. Price $0.50. New York, 1946.

various conditions." The word "conditions" is used throughout the book in an unusual sense, meaning food and other materials from which the body is constructed, as well as the states of the environment (such as temperature) in which the organism lives. With the word "conditions" so understood, is this definition anything more than a restatement of the view held by the "Mendelian-Morganians" that heredity determines the responses of the organism to its environment?

The chromosome theory of heredity is declared a fraud: "The cytogeneticists draw a picture of fertilization by looking through a microscope at a slide on which lie cells in various stages of development. They draw all they can see, and what they can not they invent by making guesses in accord with the Mendelian-Morganian theory of heredity." To suppose that heredity is transmitted through chromosomes "is completely unacceptable to a biologist. It does not agree with the sexual process, and in general is unlike any biological process taking place in the living body." On the other hand, it is acceptable to the author, but probably to few other biologists, to think that when sex-cells unite "they assimilate each other," this assimilation being alleged to be the essence of the sexual process.

Having thus administered a coup de grace to the chromosome theory, the author proceeds to tackle Mendel's laws. He is generous enough to admit that segregation of characters has been found in many hybrids, and "this was observed also in Gregor Mendel's experiments with crossing peas." But to think that this segregation should occur in a ratio of 3:1 "is basically wrong even for pea hybrids, including the factual material obtained by Mendel himself. The progenies of different hybrid plants varied even in Mendel's experiments much beyond the ratio 3:1. Thus, in the offspring of one plant there were 19 yellow and 20 green seeds, and of another plant—only a single green for 30 yellow ones." The author seems to be completely unfamiliar with the theory of probability, which explains such deviations from theoretically expected ratios, and he is surely unaware of the fact that several years ago an eminent authority on biological statistics made an analysis of Mendel's numerical data and found that they fitted Mendel's theory better than might be expected from the operation of chance. Or, perhaps, the author would regard this as clinching his argument for the rejection of Mendel's theory? In one of his published speeches, the author asserts that he challenged "Mendelian-Morganian" geneticists to produce data showing 3:1 segregation in any species, and that such data do not exist. He has not consulted the early genetic literature which is replete with such data. Nowadays, it is true, the regular 3:1 segregations often seem too trite to warrant publication. However, students taking elementary courses of genetics including laboratory exercises usually have the opportunity to observe such segregations in their own materials.

Lamarck Redivivus

In this book, Lysenko does not confine himself to debunking "Mendelian-Morganian" genetics; he offers what he thinks is a new theory of heredity, albeit many readers will recognize in this theory some nineteenth century notions, long ago and for good reasons discarded in scientific biology. He starts with the statement that a living body builds itself "from the conditions of the external environment," which it "assimilates" and transforms into itself. Now, the organisms "selects" from the environment the material it needs, and this selectivity is determined by its heredity. So far, so good. But then we read that "the external conditions, when they are included within, assimilated by the living body, become thereby internal, and not external, conditions, i. e., they become particles of the living body, and for their growth and development they in turn demand that food and those conditions of the external environment, which they

were themselves in the past." "The growth of separate parts and granules of a living body requires the same conditions of the external environment, through the assimilation of which the organism originally built these parts and granules of its body." The body or its organs, cells and "granules" may fail to find in their environment the materials ("conditions") which they "select" for their reproduction. If so, these organs may either fail to develop altogether, or they may be "forced to assimilate qualitatively or quantitatively unusual conditions." And when a "body assimilates, perforce, certain elements of the soil solution which it received for the first time, these elements become biochemically included in the body, and now become necessary for the growth and the development of the altered body." In such a way, "alterations of the living conditions which enforce changes in the development of plant organisms are the cause of alterations of heredity." Lest some readers suspect that the sentence just quoted is a sheer *lapsus calami,* it must be emphasized that the notion expressed therein is repeated over and over again, and, as a matter of fact, represents the central idea of the book under review.

One more quotation will suffice to make this abundantly clear: "the elements of the non-living nature from the environment which surrounds the plant are transformed into integral parts of the living body through a frequently forced assimilation, become living elements, and acquire the property of heredity. The developing living body will in future generations require these external conditions for the reproduction of its like."

It is incredible that as late as 1944 anyone could not only urge acceptance of the above "theory of heredity" but also delude himself into believing that this theory has in it the slightest bit of novelty. For it is nothing more than a naive Lamarckist creed, reminiscent of some nineteenth century versions of Lamarckism, particularly of that of Spencer, yet, alas, devoid of Spencerian finesse. Without being told so, we are being urged by the author to believe that acquired characters are inherited, and, more than that, that the inheritance of acquired characters is the basis of all genetics, if not of biology. Now, anyone who wishes to know why the hypothesis of the inheritance of acquired characters is discredited in modern biology can find the answers in elementary textbooks. In short, the discrediting is due to the fact that all critical experimental tests have given negative results, that all supposed indirect evidence can be better accounted for in other ways, and to the sterility of Lamarckism as a working hypothesis.

Darwin's Pangens "Rediscovered"

As if to make things more peculiar still, the author and his followers consider themselves Darwinists and carry their devotion to Darwin's name to a point bordering on fetishism. Indeed, Darwin receives some deep obeisances in the book under review, whereupon Darwin's theory of natural selection is quietly ignored. This does not prevent Lysenko from discussing in detail another of Darwin's theories, namely the "provisonal hypothesis of pangenesis," and this without acknowledging Darwin's authorship. According to Lysenko, every body part produces substances (pangenes?) which eventually become lodged in the sex cells and become integral parts of the latter. "The degree of the transmission of changes depends on the degree to which the substances of the altered body part are included in the chain of processes which lead toward the formation of the reproductive sexual or vegetative cells." Now, Darwin himself had no illusion as to the strength of the evidence on which the hypothesis of pangenesis rested, as the adjectve "provisional" clearly shows; and this hypothesis was demonstrated to be invalid already in the nineteenth century. All of which is either unknown to the author or else is ignored by him.

The Experimental Basis

Lysenko's proposal amounts, then, to no more and no less than scrapping of

all genetics and much of general biology built during the last half-century or more, and adopting a homespun concoction of Lamarck and Spencer, with some Michurin added for flavor. The least conspicuous place in the book under review is occupied by presentation of the facts which should induce modern biology to adopt so startlingly reactionary a course. More precisely, no facts at all are presented in such a way that one could critically evaluate them, but several astonishing experiments of the author and his disciples are casually alluded to. Thus, seeds from fruits of a white-fruited tomato variety grafted onto a red-fruited variety are said to have produced some red-fruited plants. The progeny of the latter is said to have "consisted of a majority of red-fruited plants, but approximately 20%-30% of the plants had white fruits." Such "vegetative hybrids" are said to have been obtained between tomato varieties with two-chambered and with many-chambered fruits, between prostrate and erect varieties, early and late varieties, etc. The offspring of a tomato grafted onto a nightshade is said to have changed in all characters, acquiring, among other things, "much improved taste qualities" and an early maturity. The author assures us that "after making such experiments any geneticist still believing in the correctness of the foundations of Mendelism will see not only the incorrectness of this theory but its harmfulness in practical application of breeding and of seed management." It is as easy as this.

"Many genetically spring varieties have been derived from winter vareties" of wheat by means of nothing more complicated than growing them under different temperature conditions. "We have already obtained several winter varieties of wheat and barley from spring varieties through education, through the influence of the external environment. These forms are no less winter resistant, and frequently even more resistant than the most resistant varieties known." This "education" consists in exposing spring varieties to low temperature for several generations. It is no wonder that having obtained such results the author entitles one of the chapters in his book "The liquidation of the conservatism of the nature of organisms." As a fitting climax, the following experiment is mentioned. "At the I. V. Michurin Central Genetic Laboratory, K. E. Enikeeva has crossed the American 16-chromosome plum 'Cheresota' with Michurin's 48-chromosome plum, variety 'Renclod Reforma.' The 16-chromosome variety was used as the maternal, and the 48-chromosome one as the paternal form. The plant obtained from the cross had the habit of the paternal form, including the 48 chromosomes. All these experiments show clearly the diversity of the biological process of fertilization which does not fit at all the cytogenetic mould invented by the Morganians." This is because "the fusion of two sex cells is a process of assimilation, a process of mutual consumption," and "depending upon which one of the sex cells assimilates its partner to a greater extent, the hybrid embryo will deviate to different degrees toward the breed of the assimilating sex cell."

Why Geneticists Scoff

What can one say about these experimental results? The author's claims are, to put it mildly, improbable. It is to be hoped that these experiments will be repeated by critical observers, and the truth or falsity of the author's claims will be established beyond doubt. The kinds of experiments mentioned in this book have all been made earlier by competent investigators, without, of course, producing the startling results alleged by the author. Thus, the offspring of grafted tomatoes and other plants never contained anything resembling his "vegetative hybrids." Winter and spring varieties of cereal crops, as well as cold resistant and non-resistant forms of cultivated and wild plants, have been extensively studied; these characters are genetically rather complex but examination of their inheritance disclosed nothing akin to the mutability claimed to

have been found by Lysenko and his collaborators.

Some people will probably wonder why geneticists do not rush to repeat these experiments. The answer is simple enough. The progress of science would be seriously disorganized if all scientists interrupt their work every time somebody publishes a dubious claim. Such claims are disposed of in due course. Admittedly, the history of science knows instances when claims first regarded as doubtful later proved to be valid and exerted an important influence on subsequent developments. One should not forget, however, that history is not as likely to record the vastly more numerous but less romantic instances when doubtful claims proved to be unfounded.

In any case, one can assert with complete confidence that genetic theories of Lysenko are invalid regardless of the final dispositon of his experimental claims. For the sake of argument, let us assume for a moment that "vegetative hybrids" are a reality, and that it is possible to obtain winter resistant wheat varieties merely by exposing their progenitors to cold. This would certainly not mean that Mendel's laws are abrogated, or that fertilization is "mutual consumption" of sex cells, or that acquired characters are inherited. Irrespective of whether the author has or has not discovered some facts of interest, his overall influence on science has been that of a thoroughgoing obscurantist. It is not Lysenko's fault that he has not succeded in wrecking genetics as well as the agricultural sciences in the U. S. S. R. We may be confident that the brilliant and active group of geneticists now working there will keep the U. S. S. R. in the forefront of scientific progress.

TH. DOBZHANSKY
Columbia University, New York

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PLANT LIFE OF THE PACIFIC

PLANT Life of the Pacific World, by Elmer D. Merrill* seems to me to be a truly amazing compendium of useful information. There is a note of authority about it which would lead any scientific man to conjecture that the author had spent many years in the Pacific area even if he did not know that Dr. Merrill grew up among its plants and built up a great herbarium in Manila. This was in the Bureau of Science, an institution for which he was largely responsible; now, alas, destroyed by the Japanese.

Dr. Merrill ranks at the top of the list of scientists who have studied the plants of the Pacific islands and his contributions to the systematic botany of that vast area are too numerous to be more than referred to here. As the Administrator of the great botanical collections of Harvard, and Director of the Arnold Arboretum his reputation is world-wide.

I am proud to have known Dr. Merrill since 1900. I met him when he was en route to Manila, on an Army transport just after the Spanish war to take up his residence in the Philippine Islands. For twenty-two years he labored there as few young men I have ever known have worked; day and night he spent in the field and among his specimens. He brought together a group of collectors and associates whose knowledge and enthusiasm has rarely been equalled anywhere in the tropical world.

As I recall him in those days, there was a fierceness about his interest in systematic botany that I came later to associate with other discoverers of new species of living creatures, insects, shells, reptiles or fossils, etc. I have sometimes wondered at the intensity of the drive

*MERRILL, ELMER D. Plant Life of the Pacific World. Pp. xv+295. $3.50. The Macmillan Co. New York. 1945.

with which such systematists are endowed—the terrific urge to cover so vast a field as that of the flora of a country with its thousands of named species and its mountains of dried specimens.

When one realizes that every living object in the landscape has a name, given it by an enthusiastic systemist, and belongs to a genus and family of an ordered classification, one can be almost appalled by the audacity of these zealots. To know those names and to apply them to the proper organisms requires a very different type of mind from that of the person to whom all plants look more or less alike. I knew a brilliant Harvard chemist who was reported to have remarked that to him the whole world of plants was nothing but a green blur. Perhaps the geneticists will identify a special gene for this odd faculty.

Anyone perusing this "guide" to the plants of the Pacific islands cannot fail to be amazed at the range of species handled—all in a familiar way—and to admire the sure grouping of them according to their ecological nitches as plants of the seashore, of the mangrove forest, of secondary forest, or grasslands, or primary forest. Noteworthy plants of special interest, weeds, cultivated plants, jungle foods are in separate chapters. Problems of the Malayan and Polynesian plant distribution; local plant names; notes on specific islands and island groups; notes on botanical history, exploration and bibliography; directions for preparing botanical specimens are all included. And there is also included the botanical arrangement of the hundreds of species mentioned, together with a glossary and complete index. Another feature of the book should draw high praise from all who use it: the 253 text figures. These help immensely in the identification of the species to which the text refers.

The book contains such an amazing fund of data that I shall use it as a reference work, even though the information regarding each species is necessarily fragmentary. What is said is so clear and so documented as to enable one to consult most readily the sources of the author's information.

One feature impresses me strikingly. The descriptions, often in technical words, are so clear cut that each species mentioned seems to have been in his hand as he wrote. They have the brilliant clearness of the remarks I have so often heard collectors make as they examine some dried specimen and the place and circumstances under which it was collected are brought vividly back to their minds.

This quality is no mere accident. This book was written by the very person who described thousands of the plants of the Pacific, and who knows at first hand practically all in this book. It is his own personal painting of the Plant Life of the vast island groups of the Pacific. It deserves the careful study of every one who wishes to comprehend the complex conditions of life in that fascinating part of the tropical world. It should be in all libraries which make any pretense of adequate coverage of the scientific literature.

The war has opened up to travel thousands of coral and volcanic islands which in my day required months to visit. Now they are only a few hours by air from our winter firesides and are destined to be visited and perhaps lived in by coming generations of Americans.

I predict this book will be the *vademecum* of all air voyagers to that region who travel to inform themselves and not merely for amusement.

DAVID FAIRCHILD.

SYMBOLS FOR HUMAN PEDIGREE CHARTS

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THE recent article in the JOURNAL OF HEREDITY by Madge Thurlow Macklin[2] has stimulated me to report on the experience of the Heredity Clinic of the University of Michigan* in the drawing of pedigree charts. Many of the symbols we use are the customary ones used by geneticists, but we have found several additional symbols to be useful! The new symbols here described have been suggested by various members of the Clinic staff and all the members of the staff have been concerned in testing them out.

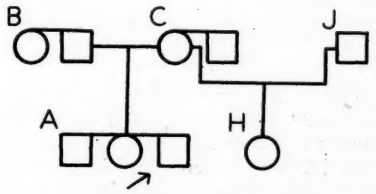
Marriages

A marriage between two individuals is indicated most simply by a straight line directly connecting their symbols, rather than by the somewhat more customary two dropped lines connected by a horizontal line. The dropped lines are subject to confusion with descent lines. Also they occupy a needless amount of space. Where dropped lines must be used to indicate a marriage between individuals whose symbols are located in distant parts of the pedigree chart the marriage line should originate from the side rather than from the bottom edge of each symbol concerned (Figure 2).

Consanguineous marriages are in our opinion much better indicated by the conventional double marriage line (Figure 3*A*) rather than by the zigzag line proposed by Dr. Macklin. Furthermore, this indication of consanguinity should in our opinion be given for any known degree of relationship between the parents. A recessive character might reappear after skipping a considerable number of generations.

An illegitimate union is best indicated by the customary dotted line connecting the symbols of the individuals concerned.

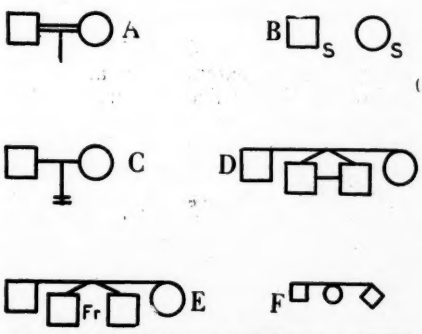
An individual who has never married may be indicated by the letter *S* placed beside his symbol (Figure 3*B*). An un-



PARENTS AND A SIBSHIP

Figure 2

Method of designating marriages and resulting offspring in a pedigree. The propositus (central figure in the pedigree) is designated by an arrow.



CONSANGUINEOUS MARRIAGES, ENDS OF LINES, AND TWINS

Figure 3

A—Consanguineous marriages designated by double marriage line. *B*—A person who never married. *C*—A marriage from which no children were born. *D*—Identical twins. *E*—Fraternal twins. *F*—Symbols for children living less than one day.

*The work of the Heredity Clinic has been supported by research grants from the Horace H. Rackham School of Graduate Studies.

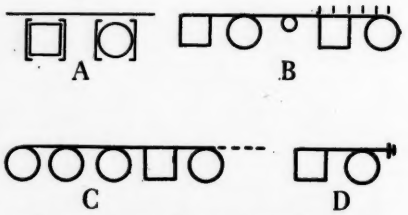
productive marriage is sometimes indicated by the words "No issue" placed below the marriage line. A shorter symbol is needed and I suggest for this purpose that a short horizontal double line be placed across the lower end of the line pendent from the marriage line (Figure 3*C*).

Generations

The several generations in a pedigree customarily are designated by Roman numerals. Although the generation is of some interest in genealogy it is of little importance in human genetics. The sibship, on the other hand, is of very considerable importance for the analysis of human inheritance and especially for statistical treatments. We therefore find it necessary to label the sibships rather than the generations.

Sibships

For labelling the sibships in a given kindred, capital letters placed at the left end of the sibship line (Figure 2) have proved to be very satisfactory. These letters are most conspicuous if they are made with red pencil or with red ink. The sibship containing the propositus is labelled *A*, and the propositus himself is indicated by the conventional arrow. The sibship containing the father of the propositus is given the letter *B*, and that of the mother the letter *C*. The sibships of the paternal grandfather, paternal grandmother, maternal grandfather, and maternal grandmother, are labelled *D*, *E*, *F*, and *G*, respectively. No particular order is followed in lettering the other sibships in the kindred. In our practice we use no letter beyond L. If there are more than twelve sibships to be lettered in a single kindred, we use two-letter combinations. It is assumed that if only single letters are used they represent actually AA, AB, AC, and so on, to AL. In starting the two-letter combinations therefore, we begin with BA, and carry on with BB, BC, and so on, to LL. Rarely will it be necessary to use three-letter combinations. The reason for not using letters beyond L is to make it easier to record these sibships on



ADOPTED CHILDREN, BIRTH ORDER, COMPLETED SIBSHIPS

Figure 4

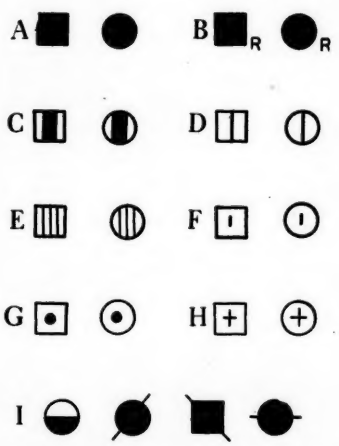
A—Adopted children. *B*—Birth order of last two children uncertain. *C*—Possibly other children in this sibship. *D*—No other birth in this sibship.

punched tabulating cards. The punched cards have only twelve spaces in each column.

In the conventional pedigree chart the symbol for each member of a sibship is shown pendent by a short straight line from the horizontal sibship line. These pendent lines seem to be wholly superfluous The chart can be made much more compact by placing the individual symbols directly below and in contact with the sibship line (Figure 2). The pendent lines have always seemed to me too much like the ropes at a hangman's party! Only for twins or other multiple births should the symbols be connected by short lines to the sibship line. Where names or numbers are given for the individual members of a sibship, these may, if desired, be written directly above the sibship line, where they will not be in the way of dates of birth and of death and of other information placed below the symbols.

Twins and Other Multiple Births

As already indicated, the symbols for the members of a pair of twins or of any other type of multiple birth are connected by short converging lines to the sibship line. All the lines from the symbols indicating the individuals produced at one multiple birth should connect to the same point on the sibship line. It is not practical for us to follow the recommendations of Macklin[2] and of Ashley[1] that the symbols for members of



RELIABILITY OF ASCERTAINMENT

Figure 5

A—Examined professionally and definitely diagnosed as showing the trait specified. *B*—Examined professionally; showed the trait specified at one time, but now repaired or recovered (as for instance, sixth digit removed). *C*—Not examined; reliably reported by non-professional observer to have the trait. *D*—Not examined; dubiously reported to have the trait. *E*—Examined professionally; did not exhibit the trait. *F*—Not examined; reported not to have the trait. *G*—Examined professionally; did not show the trait, but is a carrier of it, as shown by descendants who do. *H*—Died before the usual age of recognition of the trait. *I*—Symbols indicating various expressions of the trait.

multiple births who are not identical to one another should be independently connected to the sibship line. Our working charts must often be drawn up before it is known whether the members of a particular pair of twins are identical.

We follow the conventional method of indicating identical twins by a short horizontal line connecting their symbols (Figure 3*D*). There is only a slight possibility of this line ever being confused with a marriage line. Fraternal twins are indicated by the letters "Fr" placed between their symbols (Figure 3*E*). When it is uncertain whether or not the twins are identical, the space between their symbols may be left blank or a question mark inserted.

Miscarriages and Stillbirths

We are entirely in agreement with Dr. Macklin that the sex of a stillbirth or miscarriage should be indicated, wherever it is known, by a small square or small circle. The small diamond (Figure 3*F*) seems to be an adequate symbol to represent unknown sex, whether or not the miscarriage might have occurred at too early a date for the sex to have been identified with certainty. It is our practice to include under the heading of stillbirths and miscarriages all those individuals who did not survive longer than one day after birth. We find it impracticable to attempt to distinguish between those infants who are born dead and those which live only for a few minutes or for a few hours.

Adoptions

Adopted children often must be indicated as part of a sibship, because the other members consider them part of the family and because they bear the family name. An adopted child may be indicated very simply by brackets placed on each side of his symbol (Figure 4*A*).

Unknown Order of Birth

Frequently the order of birth of one or more members of a sibship is unknown, or it may be desired to place one or more of the members out of order to simplify the construction of the chart. To indicate an unknown or incorrect serial order of birth in a sibship a series of short vertical marks may be placed above the sibship line directly over the symbol or symbols for the individuals concerned (Figure 4*B*).

Incompletely Known Sibships

A symbol is needed to indicate those sibships which are incompletely represented on the pedigree chart. Often it is known or suspected that a sibship contains additional unreported individuals. For this indication I suggest the use of a dotted or dashed line continuing either to the right or to the left or in both directions from the horizontal sibship line (Figure 4*C*). Should it be believed on the basis of the best informa-

tion obtainable that all the members of a sibship ever born are recorded on the chart, this fact can be indicated by two short vertical lines across the right end of the sibship line (Figure 4*D*).

Individual Numbers

Each individual in a pedigree must be given a number or some other arbitrary designation, because names sometimes are repeated within a kindred. In an institution where a considerable number of kindreds are under study at the same time, as in our Heredity Clinic, it also is important not to duplicate the individual numbers in different kindreds, for sometimes it will later be proved that two kindreds initially considered separate actually are interrelated. We therefore number the individuals serially as they are received, without respect to their kindred affiliations. The kindreds also are numbered serially. A complete classification in our records for any particular person is therefore given by the combination of his kindred number, sibship letter, and individual number, for example, "561-BB-7353." In a published pedigree chart it is simplest to refer to an individual by his sibship letter together with his number in order of birth in the sibship, for example, "A-7."

Reliability of Ascertainment of the Character

In many published pedigree charts little or no distinction is made between the individuals ascertained by a proper professional examination to exhibit the trait in question and those merely reported by some member of the family to have been affected. It is even worse that often no distinction is made on the chart between those individuals reliably ascertained to be free from the trait and those about whom no information at all is available. In our opinion the ascertainment of the absence of the trait deserves as much prominence on the chart as proof of its presence. Also we think that some indication should be made on the chart of the basis for the diagnosis given.

For these reasons we use the solid black symbol (Figure 5*A*) on our Heredity Clinic charts only where the individual represented has been shown to have the trait in question by a professional examination accepted as adequate by the geneticist. Should the individual have recovered from the difficulty either by repair, treatment, or natural causes, the letter "R" is placed beside his symbol (Figure 5*B*). A symbol marked with black only in its up-and-down middle section, leaving the sides white (Figure 5*C*), indicates an individual who has not been examined professionally for the presence of the trait in question, but who has been reliably reported by a non-professional observer to be affected. If the report is considered dubious, only a narrow vertical line is placed in the middle of the symbol (Figure 5*D*). An individual who has been professionally examined and found to be free from the defect in question is indicated by three narrow vertical lines through his symbol (Figure 5*E*). An individual who has not been examined, but who is reported by a non-professional observer to be normal, is marked by a short vertical line in the middle of his symbol (Figure 5*F*). Should an individual reported to be normal or to be dubiously affected later be examined and found to be actually either normal or affected, no erasures are needed to make the proper correction on the chart.

An individual examined professionally and shown to be free from the defect in question, but who is known to be a carrier of its heredity, is marked with a black spot in the middle of his symbol (Figure 5*G*). A black cross (plus sign) in the middle of a symbol (Figure 5*H*) indicates an individual who died before the usual age of recognition of the trait. A symbol which contains no marks indicates that no information is available about the presence or absence of the trait in that individual.

More Than One Trait

The presentation of more than a single trait on one pedigree chart tends to produce confusion. If the kindred under consideration exhibits several different

hereditary traits, is it usually best to draw a separate chart for each trait. Sometimes, however, a trait is variable in its expression, and it is then important to indicate which facies is exhibited by each individual. Should there be but two known alternative facies of the trait, one may be indicated on the lower half of the individual symbols and the other on the upper half. Another way is to draw heavy black lines through the center of the symbol, extending them a little beyond its margins on each side. By using two diagonals and one horizontal line, three different expressions of the trait may be represented (Figure 5*I*). Many other possible special symbols may be employed as needed and still show the degree of reliability of ascertainment of the principal trait in question.

Drawing Guides

For marking the individual symbols on the pedigree chart we find a drawing guide to be a great time saver. A drawing guide can be constructed from any celluloid ruler or other piece of celluloid by boring holes of the proper size and then transforming certain of these holes into squares and diamonds. A fairly heavy piece of celluloid is best, because a thin piece is difficult to work without tearing. The holes are easily bored on a drill press. Several of the holes may then be shaped with a fine file or with sandpaper into the desired squares or diamonds. It would be a great service if some manufacturer would produce such a drawing guide for the use of geneticists.

Literature Cited

1. Ashley, Lawrence M. *Jour. Hered.* 36: 295-296, 1 fig. 1945.

2. Macklin, Madge Thurlow. *Jour. Hered.* 36: 222-224, 1 fig. 1945.

THE QUESTION OF DR. VAVILOV'S DEATH

THE October 5 issue of *Science* (pp. 357-8) carried a letter from Dr. Anton Zhebrak of the Timiriazev Agricultural Academy of Moscow. This was in the nature of a reply to an earlier letter by Dr. Karl Sax of Harvard University which raised questions of political interference in research, with rather pointed emphasis on the Lysenko controversy. Sax named three workers who took a prominent part in opposing Lysenko, and who had not been heard of for many months. These were Navachin, Karpetchenko, and Vavilov. Zhebrak insisted that genetic research in the U.S.S.R. is wholly free and untrammeled. He cited recent publications of Navachin, but he made no mention of Vavilov and Karpetchenko.

In the December 21 issue of *Science* (p. 649) Dr. Sax raised some further questions. Regarding Dr. Vavilov Sax states that "We now have information from our own Academy of Sciences that Vavilov is dead. How did he die? and why?" Inquiry of the National Academy of Sciences elicits the information that no statement has been issued regarding N. I. Vavilov, geneticist. Based on an announcement in the British Journal *Discovery,* the National Academy of Sciences did announce the death of Dr. Sergi Vavilov, brother of N. I. Vavilov, and President of the Academy of Sciences of the U.S.S.R. He was an astronomer and mathematician. It appears that this is the announcement to which Dr. Sax has referred.

Recently received in the United States is a list of the members of the Academy of Sciences of the U.S.S.R., distributed to the members and guests attending the Jubilee Celebration commemorating the 220th anniversary of the Russian Academy of Sciences. This list does not contain the name of N. I. Vavilov. A member of the government mission who was in Moscow in the summer of 1945 and who met several biologists there, reports that N. I. Vavilov died in the early months of 1945, but that he obtained no information as to the circumstances of his death. This is confirmed by a story from Britain that a communication announcing Dr. Vavilov's election as a corresponding member of the Royal Society was returned with the notation that Dr. Vavilov had died early in 1945.

—R. C.

THE INHERITANCE OF ZYGODACTYLY*

KARL A. STILES and DOROTHY A. HAWKINS†

WEBBED TOES

Figure 6

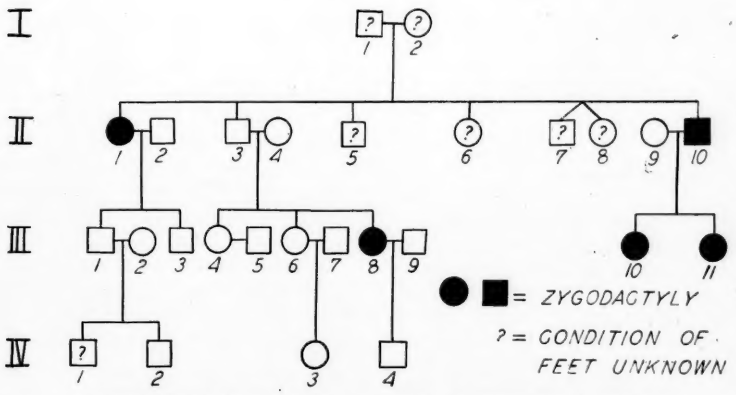
External and x-ray photographs of the feet of III-10 (Figure 7). As is usually the case the webbing occurs only between the second and third toes. The bones are normal.

THE term zygodactyly was coined by Weidenreich[11] to designate simple webbing between the toes, in distinction to syndactyly, wherein fusion or abnormality of the bony structure of hands or feet is involved.

The webbing characteristic of zygodactyly usually involves the skin and superficial fusion between the second and third toes. Cases have been reported in which other toes have been affected, and Whitney[12] even states that complete

*Contribution from the Science Laboratories of Coe College No. 19 N.S. Aided by an academy grant from the American Association for the Advancement of Science through the Iowa Academy of Science. The authors wish to thank Doctors A. W. Erskine and J. V. Prouty of Cedar Rapids, Iowa, for their kindness in the preparation of the excellent X-ray film and for other assistance.

†Michigan State College, East Lansing, Mich.; and Coe College, Cedar Rapids, Iowa, respectively.



INHERITANCE OF ZYGODACTYLY PEDIGREE CHART

Figure 7

A pedigree chart showing the inheritance of zygodactyly. Although II-3 does not show the trait himself, one of his daughters (III-8) does. This suggests irregular dominance as the mode of inheritance. All individuals indicated by the symbols (? inside a square or circle) are dead, making it impossible for the authors to learn the condition of their feet.

webbing of all toes has been reported. The extent of the webbing varies considerably, in some feet it extends to the first joint of the toes, in others to the second joint, and in still others to the tip of the toes.

Usually both tendons and bones are normal in structure, though Weidenreich[11] reports an instance of partial fusion of the tendons, the tendons for the extensor digitorum longus muscle of the toes being split farther up between the third and fourth and fifth toes than between the second and third. This observation supports Schultz's,[9] view that there appears to be greater unity between these toes. Schurmeier,[10] in a study of twenty thousand drafted men, found only eight who possessed this trait, the webbing always being between the second and third toes.

The embryogeny of webbed toes has been studied by Schultz[9] and Minot.[6] It is normal for the toes to be webbed when the fetus is from five to six weeks old. The webbing appears to be slightly more extensive between the second and third toes. As the digits develop and elongate, the webbing becomes less noticeable and the toes by the end of the eighth week become separated, that is, the webbing is reduced to mere remnants at the base of the digits. But in the case of zygodactyly the webbing grows outward with the toes instead of becoming reduced. It can be seen from the embryogeny of webbed toes that zygodactyly appears at birth, but it does not increase as the individual grows older.

Description of Pedigree

An examination of the pedigree (Figure 7) reveals that five individuals possess this trait. In every case the webbing extends only to the first joint, and it in no way impairs the function of the toes. Because I-1 and I-2 are dead the condition of their feet could not be determined; however, it is probably safe to assume that one of them either showed the trait or was a "carrier" of it. II-1 possesses webbed toes. She married a man with normal toes and neither of their sons, III-1 or III-3, shows the trait. III-1 married a woman with normal toes and their son, IV-2, has normal toes. IV-1 died at birth. Although II-3 does not exhibit zygodactyly, one of his three daughters, III-8 does. But her

son does not show any such anomaly. II-10, who exhibits the trait, married a woman with normal toes. However, both their daughters, III-10 and III-11, have webbed toes.

Discussion

A similar report of zygodactyly by Schofield[8] seems to indicate that this character is inherited as a Y-Chromosome trait. Schofield[8] states that in the family he studied the character always appears in the male sex. It is transmitted only through the male lineage and it is never concealed in the males of this lineage. Thus it appears to be inherited as a secondary sex character. On the other hand, Guyer[2] reports a pedigree which clearly indicates a sex-linked inheritance. In addition to these papers Castle,[1] Hurlin,[4] Schultz,[9] and Wright[13] seem to agree that the anomaly, webbed toes, depends on the transmission of a dominant gene located in one of the autosomes. Heston[3] and Scheinfield[7] suggest that although the trait is due to a dominant gene its expression is modified by other genes.

In view of the fact that zygodactyly occurs so infrequently, eight in twenty thousand, it appears highly improbable that affected individuals in the pedigree presented here would marry persons heterozygous for the trait. There is no evidence that the character is sex-limited or sex-linked, for it appears to be inherited by males as well as females. The condition is probably governed by a single dominant autosomal gene; but, if so, its expression may be influenced by other genes, as it is not always manifested phenotypically. Manson[5] observed varying degrees of expressivity of the character; this is thought to be the result of modifying genes. In his paper he stated that while the mother was but slightly affected, her children showed the anomaly in an augmented form. Evidently somatic manifestation may not indicate the intensity of the hereditary factor present in the germ cells.

In the case here reported there is evidence that the expression of zygodactyly may be influenced by modifying genes, for II-3 who does not exhibit the trait has a daughter III-8 who does. This suggests that II-3 is a "carrier" of the zygodactyly gene, as it is highly improbable that his wife is a "carrier" in view of the infrequency of the occurrence of this gene in the general population. If these assumptions are correct, then the mode of inheritance is irregular dominance.

Summary

1. The web between the toes of the zygodactylous individuals of the pedigree reported extends only to the first joint of the toes, and occurs only between the second and third digits on both feet.

2. The pedigree reveals that this trait is conditioned by a dominant gene with reduced penetrance. Persons presumably heterozygous for the trait sometimes have normal toes.

Literature Cited

1. Castle, W. E. *Jour. Hered.* 14:209-210. 1923.
2. Guyer, Michael F. *Being Well-born.* Pages 121-122. Bobbs-Merrill Co.
3. Heston, W. E. *Jour. Hered.* 23:379-381. 1932.
4. Hurlin, Ralph G. *Jour. Hered.* 11: 334-335. 1920.
5. Manson, J. S. *British Med. Jour.* No. 3857, page 1044. 1934.
6. Minot, C. S. *Human Embryology.* Pages 391-392. William Wood Co. 1892.
7. Scheinfeld, Amram. *You and Heredity.* Pages 141, 187, 196. Frederick A. Stokes Co. 1939.
8. Schofield, R. *Jour. Hered.* 12:400-401. 1921.
9. Schultz, A. H. *Jour. Hered.* 13:113-117. 1922.
10. Schurmeier, H. L. *Amer. Jour. Phys. Anthropol.* 5:51-60. 1922.
11. Weidenreich, F. *Zeitschr. f. Morphol. u. Anthrop.* 22:51-282. 1921.
12. Whitney, David D. *Family Treasures.* Page 182. The Jaques Cattell Press. 1942.
13. Wright, S. *Jour. Hered.* 13:118. 1922.

THE MULTINIPPPLE TRAIT IN SHEEP

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EWES having more than two nipples are observed frequently. Attention was focused on this trait as a result of work by Alexander Graham Bell. Those results were summarized in the JOURNAL OF HEREDITY by Bell.[3] Some observations on his data were also reported in this journal by Castle.[5] Descendants of the Bell flock were used in further experimental work and the object of this report is to give a summary of recent findings concerning this trait and its possible importance in sheep breeding.

History of Bell's Multinippled Sheep

Dr. Bell began sheep breeding in 1889, and detailed records on the occurrence of the multinipple condition in his flock date from 1890. His original flock of multinippled animals was apparently selected from nondescript farm flocks in the vicinity of his farm at Beinn Bhreagh, near Baddeck, Nova Scotia, and the mutton and wool qualities were not well developed. This observation is borne out by Miller,[10] who used a multinippled ram descended from Bell's flock on improved ewes at Rothamsted, England, and discarded the lambs because of their inferior characteristics. Bell's flock was broken up in 1914, but part of it was continued by Mrs. Bell, and Mr. Bell supervised it and another part of the flock, owned by a neighbor, until June, 1922, shortly before Bell's death, Details of the management and breeding are given by Bell.[2, 3]

The New Hampshire Agricultural Experiment Station obtained one ram and five ewes from Mrs. Bell's flock in 1923 and eight more ewes in 1924, according to Kendall.[8, 9] This station had already developed a flock of Southdown × Rambouillet sheep and also a group of Rambouillet × Oxford sheep, but most of the latter cross had been discarded. Multinippled rams were mated to ewes of the two crosses in an attempt to combine desirable mutton and wool characteristics and the multinipple trait. Later on, many of the ewes descending from this cross were mated with a four-nippled Suffolk ram to give the sheep the appearance of a standard breed. This work is described by Ritzman.[14,15]

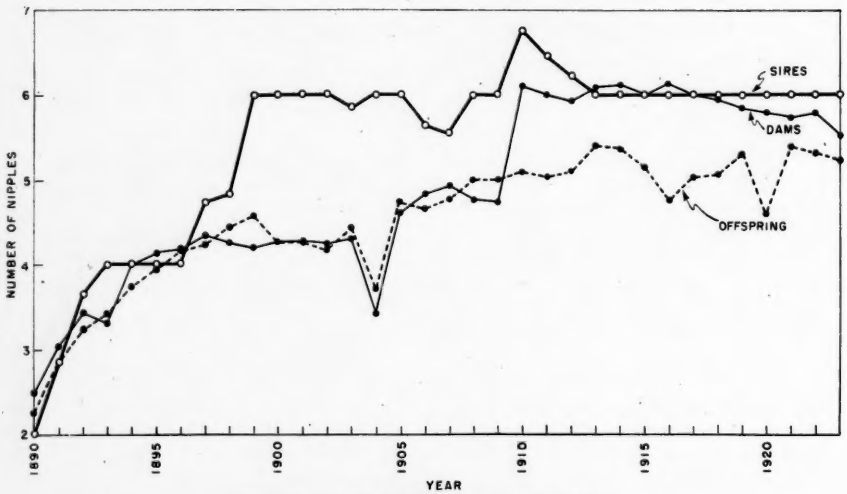
Most of the flock thus formed at the New Hampshire station was purchased by the United States Bureau of Animal Industry and transferred, late in 1941, to the United States Morgan Horse Farm, Middlebury, Vermont. The transferred animals included 15 females of approximately ½ Bell multinipple, ¼ Southdown, and ¼ Rambouillet blood (referred to later as "B" animals), and 46 females of approximately ½ Suffolk, ¼ Bell multinipple, ⅛ Southdown, and ⅛ Rambouillet blood (the "SB" animals). Most of these ewes had four nipples and some had five or six. Rams used on this flock at Middlebury had four nipples, except one with five nipples. These included two SB rams, one Suffolk ram that had been used in the flock at New Hampshire, two rams resulting from mating the Suffolk to SB ewes, one ram resulting from mating a B ram to an SB ewe, and two Columbia rams.

Data obtained during three years of breeding work at Middlebury, Vermont, from the immediate ancestors of the sheep in the Middlebury flock (New Hampshire Agricultural Experiment Station data), and from the original Bell flock through data tabulated by Bell,[1] and summarized by Castle,[5] were used in a study of the multinipple trait. This was reported in detail by the authors[12] and a brief summary is given below.

Experimental Results

Inheritance of the trait

Bell's objective in the breeding of multinipple sheep was to develop a type that could produce two or more lambs at a



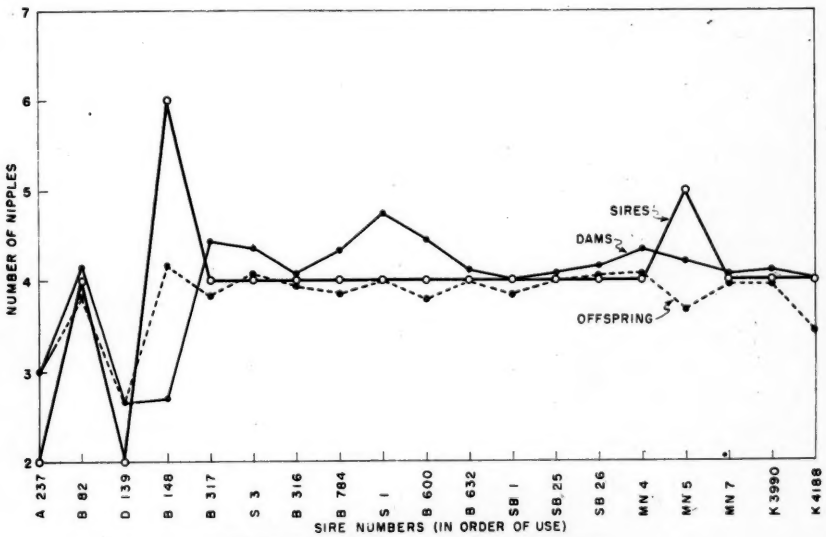
INCREASE IN NUMBER OF NIPPLES

Figure 8

Number of nipples reported in sires, dams, and offspring in Dr. Bell's flock. The number of nipples was rapidly increased to an average of four, after which progress was much slower. After 1910, even though six-nippled parents were used almost exclusively, the number of nipples of the offspring was not consistently increased to an average of over five. Data from Castle.[5]

birth and have adequate milk on which to raise them. He selected for extra nipples with the idea that an increase in the number of functional nipples would result in an increased milk supply for the lambs. Selection for extra nipples was quite effective in Bell's flock, as may be seen from the graph shown in Figure 8. Castle[5] examined the data and concluded that the character, supernumerary nipples, is highly inherited, but no detailed analysis was made to determine the mode or degree of heritability. Examination of Figure 8 reveals that it was possible, by selection of rams and ewes, to increase the average nipple number in offspring to approximately four in a comparatively short time. Progress beyond this point was much slower even though rams averaging six nipples were used and the number of nipples on dams averaged six or nearly so from 1910 on.

Ritzman[14] studied the problem of inheritance further and proposed an explanation of the mode of inheritance. It was based on the assumption that additional gland development was correlated with a large number of nipples. The theory proposed included three pairs of genes for gland development and three pairs for nipple number. These genes might be present in varying "dosages" of effective alleles, and recombinations could result in offspring having various numbers of nipples and varying degrees of nipple and gland development. Ritzman reports that the theoretical and expected ratios were in rather close agreement. Popova and Kardymovich[13] also studied inheritance of the multi-nipple condition in various breeds of sheep in Russia and concluded that four nipples resulted from the presence of dominant genes *N N*, whereas ewes with two nipples had recessive alleles *n n*. Ewes with intermediate nipple development were assumed to be heterozygous *Nn*. It was assumed that *N* might not be completely dominant in all cases, and it was suggested that occurrence of more than four nipples was due to a



STABILIZATION OF NIPPLE NUMBER

Figure 9

Average number of nipples, by sire groups, on sires, dams, and offspring in the multinippled flock during more recent years. These data were obtained during 1926-1943, inclusive.

modifier *En,* which operated only in the presence of *N*.

The numbers of nipples on sires, dams, and offspring in the flock at the Morgan Horse Farm and on part of this flock and its ancestors at the New Hampshire station are shown in Figure 9. The rams are placed in the chart in order of the first year in which each was used. The periods of use ranged from one to six years. Ram A 237 was first used in 1926 and rams K 3990 and K 4188 were used in 1943. During the early years at the New Hampshire station, the average number of nipples in the flock dropped below the level established by Bell, owing to crossing with types that did not possess the multinipple character. However, by selection of rams and ewes possessing four or more nipples, the number on the offspring was soon stabilized at approximately four.

An analysis of variance was made with the 668 progeny included in Figure 9 to determine the significance of differences between sires in the numbers of nipples on their progeny, after adjustment for numbers of nipples on the ewes to which the sires were mated. The results showed that the rams differed significantly in their ability to transmit an increased number of nipples to their offspring.

The relation between the number of nipples on the dams and the numbers on their offspring was determined on 668 dam-offspring pairs on a within-sire basis. A ewe was included once for each offspring produced. The coefficient of correlation was +0.072, and the coefficient of regression was also 0.072. Both of these figures were too small to be significant, but they approached significance at the level of $P=0.05$.

The within-sire dam-progeny correlation may be used as a basis for estimating heritability owing to additive effects of genes. The coefficient of correlation is multiplied by 2. This method and others for estimating heritability are described by Whatley.[17] In the present study the estimate of heritability was 14.4 percent. Obviously, this is only a rough approximation since the correlation upon which it is based is not suffi-

ciently large to be significant. It is rather low in view of Castle's[5] statement that the multinipple character is strongly inherited. However, the present data were obtained on a population in which the number of nipples had become stabilized reasonably well (Figure 9), whereas the data studied by Castle (Figure 8) were obtained when the nipple number was being increased by selection.

The estimates of heritability given in the preceding paragraph were based on the total number of nipples on each dam and her offspring. Data were available on 83 dam-daughter pairs, collected at the New Hampshire station and the Morgan Horse Farm, in which the numbers of functional nipples on both dams and daughters were known. The daughters were sired by 12 rams. An analysis of variance and covariance showed that the variation between offspring of rams only approached significance. The relation between dams and daughters, as shown by a coefficient of correlation, was 0.13. The coefficient of regression was 0.11. These figures are not significant. Estimates of heritability based on them would be 26 and 22 percent, respectively.

The above estimates of heritability are all based on coefficients of correlation and regression that are too small to be significant, and therefore must be regarded as tentative. However, they indicate that variations in nipple number and function were determined in part by heredity in the population studied. These estimates were obtained on a population in which nipple number has been established at a fairly high level by selection, hence estimates on animals in which the trait has not been so well fixed might be quite different.

Twinning and Milk Supply

Two important questions concerning the usefulness of the multinipple trait needed further study. These were: (1) Are the multinipple trait and the tendency for multinipple births correlated? (2) Do lambs from ewes with more than two nipples obtain more milk and therefore make faster gains than lambs from ewes with fewer nipples? In connection with the latter question, information was also needed on the structure and milk-producing capacity of the extra nipples. Information on this and related points is summarized below.

Functional capacity of extra nipples

Many extra nipples are not functional. This is illustrated by data on 106 ewes from the New Hampshire station and the U. S. Morgan Horse Farm. The coefficient of correlation between total nipples and functional nipples was 0.24, a small but statistically significant correlation. Of these ewes, the largest group (86 ewes) had four nipples. In 55 cases, four were functional, in 18 cases three were functional, and in the remaining 13 cases only the two primary nipples were functional.

The vital question, when considering the desirability of the multinipple trait, is whether an increased number of nipples is associated with a large lamb crop, an increased rate of growth in lambs, or both. Castle[5] points out that there is not necessarily any relation between number of nipples and twinning, and that there is nothing in the records on Bell's flock to indicate whether or not an increase in milk production accompanied the increase in number of nipples per ewe. Castle doubted whether variation in number of nipples would affect materially the amount of milk produced.

Relation of Age of Ewe to Fertility

The fertility of ewes varies with age. This fact was shown by Johansson,[6] who found that fertility increased up to the fourth or seventh year of age, and then decreased slightly. Kelley[7] reported that Merino ewes produced the largest percentage of twins between four and ten years of age. A similar effect of age was found in the data on multinippled sheep collected at the New Hamsphire station and the Morgan Horse Farm. The average number of lambs per birth increased up to six years of age and this trend was significant.* After six years, the level

*For significance of age differences: 2 to 6 years, $P = <0.01$; 7 to 10 years, $P = >0.05$.

of lamb production declined, but the trend was not statistically significant with the small numbers involved. Therefore, in considering the relation between number of nipples and number of lambs produced, it was necessary to study the data within each age group.

Relation between number of nipples and fertility

The relation between number of nipples (total nipples and functional nipples) on ewes and the number of offspring produced, in each age group, is shown below:

Age of ewe	Coefficients of correlation between number of young and— Total nipples	Functional nipples
2	0.11	—0.004
3	.15	—.02
4	.21*	.29**
5	.03	.06
6	.01	—.13
7	.11	.20
8	.27	.02

*Significant. **Highly significant.

The coefficients of correlation are low, and in only one group (four-year-old ewes) were the coefficients sufficiently large to be significant. Since most of the coefficients are positive, there may be some relation between the two charateristics, but it is too low to be of practical value in selection, for selection in one trait would not result in any material collateral increase in the other. Therefore, development of a multinippled flock by selection would not itself guarantee larger lamb crops. Popova and Kardymovich[13] studied this problem in a number of Russian breeds and found no correlation between fertility and an increase in number of nipples.

Relation between number of nipples and growth of lambs

The relation between number of nipples on ewes and the weaning weights of their lambs was determined, this phase of the study being based on records obtained at the Morgan Horse Farm. The weaning weights of all lambs were adjusted to a single-ewe basis, use being made of correction factors obtained from the data themselves, in accord with the results obtained and procedure suggested by Phillips and Dawson.[11] The data were analyzed in two ways. In the first analysis each ewe was included once for each offspring produced. The relationship was determined for the ewes of all ages and also for the two-year-old group. The numbers of pairs were 186 and 24, respectively, and coefficients of correlation were calculated for the relationship between total nipples and weaning weight and functional nipples and weaning weight. The coefficients were:

Group	Relation (r) between weaning weight and— Total nipples	Functional nipples
Ewes of all ages	—0.01	—0.09
Two-yr.-old ewes	—0.22	—0.10

In the second analysis, each ewe was included only once and the average of all her offspring was used in calculations. In this case the numbers of pairs were 65 and 23 for ewes of all ages and the two-year-old ewes, respectively. The coefficients of correlation were:

Group	Relation (r) between weaning weight and— Total nipples	Functional nipples
Ewes of all ages	+0.11	+0.21
Two-yr.-old ewes	—0.14	—0.07

None of the above coefficients was significant, and there is no indication of a relationship between number of nipples and growth of lambs. If any exists, it might be negative rather than positive, since six of the eight coefficients are negative. These results are in accord with observations by Popova and Kardymovich[13] on several breeds of sheep in Russia. They found no correlation between an increased number of nipples and milk yield. Bonsma[4] summarizes results of other workers on milk yield in sheep, and indicates that there is a close relation between milk yield of dam and growth of lambs. Hence, the use of growth rates as an indirect measure of

THE FOUR-NIPPLED UDDER OF EWE NO. 22-SB

Figure 10

Sections through the left half are shown in Figure 11. The extra nipples are quite large but the right and left extra nipples had capacities of only 1.27 and 2.54 c.c., respectively, while the respective capacities of the primary halves were 300.7 and 371.8 c.c.

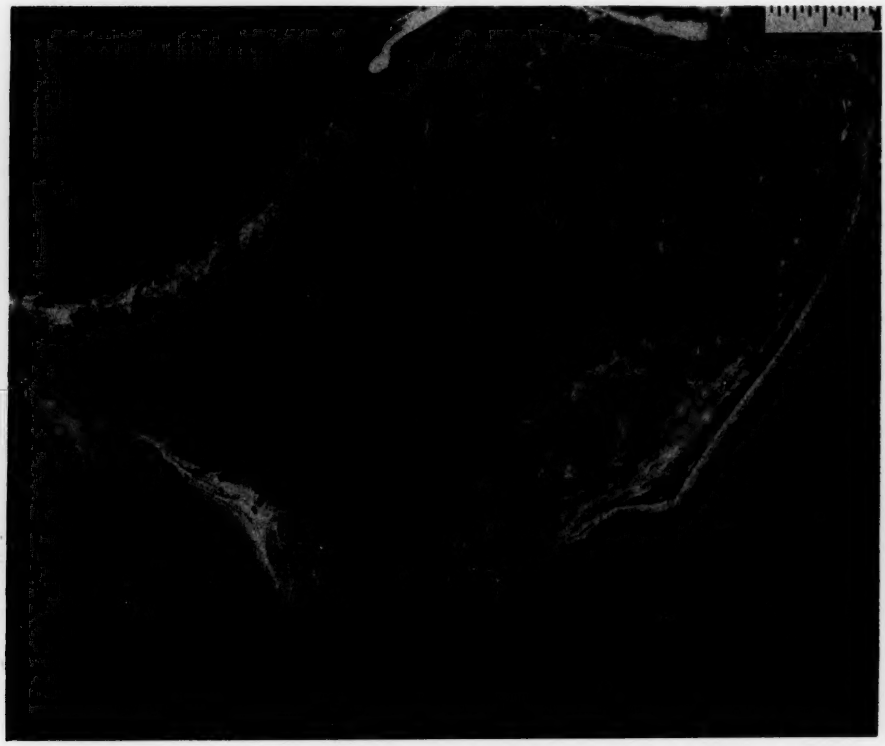
milk yield in the work described above seems justified.

One possible explanation for the results obtained in the present study is that many of the ewes had two large nipples and two or more smaller ones. Some of the young lambs were unable to nurse the very large nipples, unless given assistance, and the small nipples did not appear to yield enough milk to keep the lambs alive. In a few cases, when large nipples were not nursed, caking of the udders resulted. Another possible explanation is that as the number of nipples increased there was little or no increase in the actual amount of secretory tissue and milk-producing capacity. The studies on the udders, reported on the following pages, were undertaken to obtain information on this point.

Anatomy and capacity of udders

The udders of 48 ewes were studied to determine the capacity of all parts of the udders that had functional nipples, and the structure, particularly the relation of extra nipples to the main part of the udders. These animals included 37 lactating and six dry ewes from the multinippled flock and four lactating and one dry ewe of other types, for comparison.

Studies of udder capacity of lactating ewes were made from 10 to 13 days



SECTION OF UDDER

Figure 11

A longitudinal section through the left half of the udder from ewe No. 22-SB, a four nippled animal. Note the connective tissue wall, A, separating the extra teat cistern, B, from the main portion of the udder.

after lambing. Lambs were left with the ewes until the latter were slaughtered. Each ewe was milked out completely by hand just before slaughter. The udder was removed immediately after slaughter and mounted on a wooden frame of suitable size, so that it was in as nearly a natural position as possible. In determining the capacity, the udder was filled with a 5-percent solution of formaldehyde under pressure, which was obtained by gravity. A series of calibrated glass tubes was used as storage chambers for the fluid. The fluid was then allowed to flow through tubes into the udder, all parts being filled at once, for four minutes or for a shorter time if the udder filled to capacity and fluid began to seep out through the tissues. The glass storage tubes were all filled to the same level before injections were begun, and this level was about eight feet six inches above the point of injection at the bottom of the udder. The fluid injected into the extra nipples contained dyes of different colors so that the tissue associated with each nipple would be clearly outlined.

In all cases the capacities of the extra nipples were small. For example, in 29 four-nippled ewes studied, the average volumes of fluid injected into the right and left primary halves were 254.8 and 251.2 c.c., while the extra right and left

nipples accommodated only 3.6 and 4.6 c.c., respectively. Most of the extra nipples that had openings usually secreted a small amount of milk. But the small volume of fluid injected indicates that the milk-secreting capacity of these extra nipples was quite limited. In some of the extra nipples, much of the fluid injected had infiltrated into connective tissue, hence the real capacity is even more limited than the averages indicate. Thus, it appears obvious that lambs would receive little milk when they nursed the extra nipples, and if they nursed these in preference to the large primary nipples, as was observed in some cases, the nutrients obtained would be inadequate for normal growth.

The multinippled sheep are considerably larger than the Southdale type, and the udder capacity of the one lactating Southdale ewe studied had considerably less capacity. Karakul ewes, on the other hand are more comparable in size to the multinippled ewes and have large udders. The average capacity of the three lactating Karakul ewes studied was greater than that of the ewes from the multinippled flock. Incidentally, all the Karakul ewes used in this study had one or two extra nipples.

Swett and coworkers[16] report a significant positive correlation of 0.64 between capacity of the udder, as determined by amount of fluid injected under constant pressure, and the milk-producing ability of the cow. No comparable data are available on sheep, but on the basis of the findings with cattle, the method used in the present study seems justified.

After the udders were filled with fluid, they were frozen so that sections could be made for a study of the internal structure. Sectioning was done with a band saw. Examination of sections shows that extra nipples are not attached to the main bodies of secretory tissue in the udder. These relationships could be seen clearly in the sections where the dye injected into the extra nipples outlined the structure of them and their associated tissues. A typical section of an udder is shown in Figure 11, and the intact udder from which this section was made is shown in Figure 10.

Conclusion

The results summarized above indicate that the incidence of the multinipple trait in sheep can be readily affected by selection of sires and dams. There is little indication, however, that this trait has practical value in sheep husbandry, either by association with high fertility or increased milk production. In most flocks this character can safely be disregarded in selection but if the incidence of multinippled individuals becomes high enough to be objectionable it should be possible to bring it under control rather quickly by the use of rams with only two rudimentary nipples.

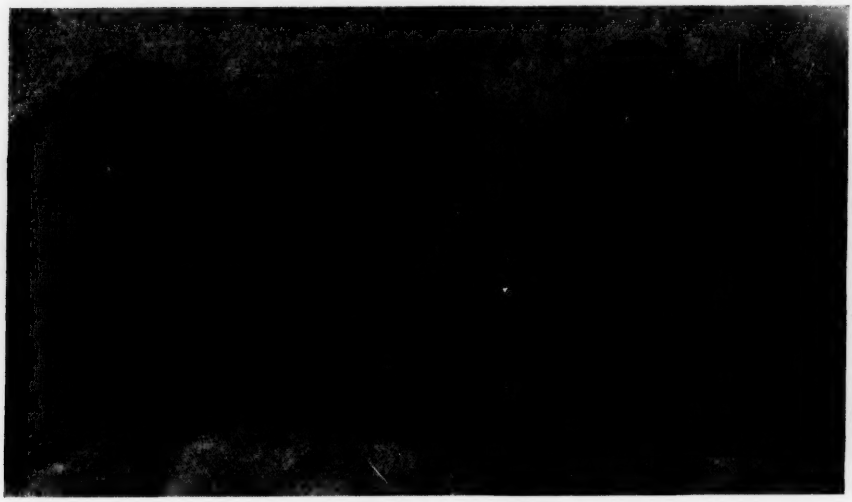
Literature Cited

1. Bell, A. G. Sheep Catalogue of Beinn Bhreagh, Victoria Co., Nova Scotia. Published by the author, Washington, 1944.
2. ————. *Science* 19 (489) :767-768. 1904.
3. ————. *Jour. Hered.* 14:99-111. 1923.
4. Bonsma, F. N. Union So. Africa, Dept. Agr. and Forestry Bul. 251, 20 pp., illus. 1944.
5. Castle, W. E. *Jour. Hered.* 15:75-85 illus. 1924.
6. Johansson, I. Lantbruksveckans Handlijger, pp. 165-188. 1942.
7. Kelley, R. B. *Austral. Vet. Jour.* 15: 184-198. 1939.
8. Kendall, C. Ann. Rpt. Director, *N. H. Agr. Expt. Sta. Bul.* 212, p. 16. 1924.
9. ————. Ann. Rpt. Director, *N. H. Agr. Expt. Sta. Bul.* 216, p. 11. 1925.
10. Miller, H. G. *Scot. Jour. Agr.* 17: 79-85. 1934.
11. Phillips, R. W., and W. M. Dawson. *U. S. Dept. Agr. Cir.* 538, 17 pp. 1940.
12. Phillips, Ralph W., Ralph G. Schott and Damon A. Spencer. The Genetics, Physiology, and Economic Importance of the Multinipple Trait in Sheep. *U. S. Dept. Agr. Tech. Bul.* No. 909. 1945.
13. Popova, E. T. and E. I. Kardymovich. *Genetics of Sheep*, pp. 63-91. (In Russian, Abstract in *An. Br. Abstracts*, 1:19. 1933.) 1932.
14. Ritzman, E. G. *N. H. Agr. Expt. Sta. Tech. Bul.* 53, 32 pp., illus. 1933.
15. ————. *N. H. Agri. Expt. Sta. Bul.* 331, 20 pp., illus. 1941.
16. Swett, W. W., F. W. Miller, R. R. Graves and G. T. Creech. *Jour. Agr. Res.* 45:577-607. 1932.
17. Whatley, J. A. *Jour. Agr. Res.* 65: 249-264. 1942.

A MOTTLED-LEAF CHARACTER IN WINTER SQUASH

Inherited as a Dominant Mendelian Character

D. H. Scott and Mary E. Riner*



MOTTLED-LEAF SQUASH

Figure 12

Winter squash leaves showing the common uniform leaf color on the left and the mottled color on the right. Mottled leaf is conditioned by a single dominant gene.

THE literature contains relatively few reports on the inheritance of genetic characters in winter squash (*Cucurbita maxima*). Hutchins[2] has shown that green color of fruit is partially dominant to blue in the Hubbard variety and that it is controlled by a single factor difference.

One of the squash varieties being used in the breeding work at the Cheyenne station has a distinctively mottled leaf. Since leaf mottling is one of the few vegetative characters that can be easily recognized, it may be of value as a marker in studying possible linkage relations. Linkage studies are not sufficiently advanced to be presented at this time, so that this report deals only with the inheritance of the mottling.

Materials and Methods

The parental lines involved in the work had been inbred from five to seven generations and appeared very uniform in type. They consisted of Buttercup, Golden Hubbard, and a variety known locally as Zuni which has the mottled character.

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The mottled effect in the leaves (Figure 12) is similar in appearance to that found in some varieties of *C. moschata*.[1] It is visible only on the upper surface of the leaves and is a result of light-colored gray-green tissue being surrounded by the darker green tissue usually found in winter squash leaves. Except for the mottled leaves the Zuni variety is typically *C. maxima*. It corresponds in leaf shape, fruit stalk type, and seed shape to *C. maxima* as described by Erwin and Haber.[1] Its complete compatibility in crosses with other varieties of *C. maxima* also indicate that Zuni is a variety of this species.

When a qualitative starch test was made on the mottled leaves, using the iodine stain method, there was no visible difference in the quantity of starch present in the gray-green tissue in comparison with the darker green tissue. Apparently the mottling is not due to a chlorophyll deficiency in the light-colored tissues.

In the genetic studies all crosses were made in the greenhouse. The following progenies were grown in the field in 1944, in addition to the parental lines: the F_1 and the F_2 of Zuni $\times$ Buttercup, the F_1 backcrossed to Zuni, and the F_1 backcrossed to Buttercup; the F_1 and the F_2 of Zuni $\times$ Golden Hubbard, and the F_1 backcrossed to Golden Hubbard.

Results and Discussion

The F_1 plants from both crosses all had mottled leaves which were similar in appearance. The leaves of all plants in the Zuni $\times$ Buttercup backcrosses to Zuni were mottled also. The segregations of the populations are shown in Table I. Both of the F_2 progenies showed a close fit to a 3:1 segregation of mottled *vs* uniform; and progenies from both backcrosses to the parent with recessive uniform-leaf character segregated in approximately a 1:1 ratio. The mottled-leaf and uniform-green-leaf characters in winter squash are controlled in inheritance by a single pair of allelic genes, and allele for mottled-leaf being dominant. The symbols *MM* and *mm* are suggested for mottled-leaf and uniform green leaf, respectively.

To summarize, mottled leaf is a distinctive vegetative character not previously described in the winter squash (*Cucurbita maxima*). It is inherited as a single dominant gene that may have value as a marker gene for use in linkage studies.

Literature Cited

1. Erwin, A. T., and E. S. Haber. *Iowa Agr. Expt. Sta. Bul. 263.* 1929.

2. Hutchins, A. E. *Amer. Soc. Hort. Sci. Proc.* 33:513-514. 1936.

Table I. Segregation in progenies for leaf color.

Progenies	Leaf color of seedlings: Mottled	Leaf color of seedlings: Uniform	Probable segregation	χ^2
	Number	Number	Ratio	Value
F_1 Zuni(mottled) x Buttercup(uniform)	99	0	---	---
F_1 Zuni(mottled) x Golden Hubbard(uniform)	97	0	---	---
F_2 Zuni x Buttercup	78	20	3:1	1.11 †
F_2 Zuni x Golden Hubbard	77	23	3:1	0.21 †
BC (Zuni x Buttercup) x Buttercup	46	52	1:1	0.36 †
BC (Zuni x Golden Hubbard) xGolden Hubbard	41	54	1:1	1.78 †

†The observed segregation is not significantly different from the calculated probable segregation.

✦

Errors in the November 1945 Issue

In article by H. H. McKinney and E. E. Clayton, "Genotype and Temperature in Relation to Symptoms Caused in Nicotiana by the Mosaic Virus," Vol. 36, page 330. First column, third line should read: "The F_1 *necrotic* plants are definitely . . .". The fourth line should read: ". . . Twenty plants *of the* F_1 *population* were inocu-".

"LONG HUSK" STERILITY IN MAIZE

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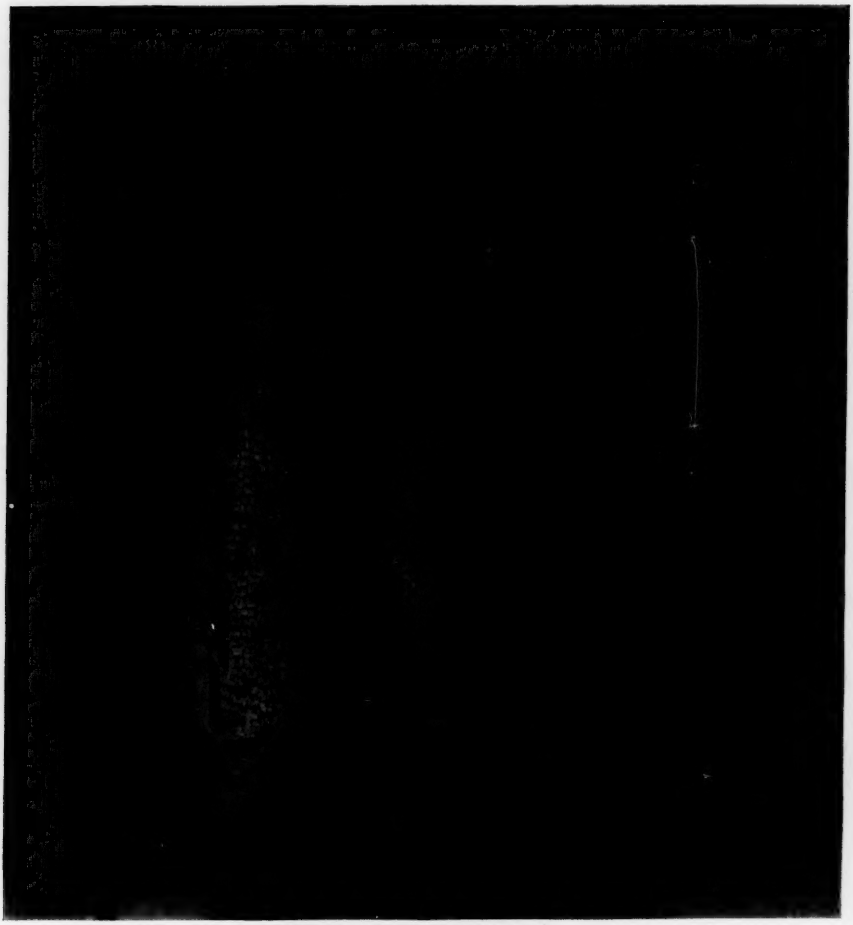

LONG HUSK STERILITY

Figure 13

Open pollinated ears of Connecticut 95 grown in an increase field in 1945. Three ears at left received ample pollen but produced no seed because of long husks. Two ears at right had husks shortened at time of pollination and produced a full set of seed. This character can only be perpetuated in artificial cultures.

AN inbred line of sweet corn, Connecticut 95, that is incapable of maintaining itself under open pollinated field conditions has been developed. It must be maintained by hand pollination or if grown under field conditions the ear shoot must be severely cut back in the way that is usually done be-

fore hand pollinations are made. Such a strain of maize could not have arisen naturally and is necessarily a product of a special hand pollination technique.

In making hand pollinations it is customary to shorten the ear shoot, cutting it back to such an extent that there is an inner core of silks showing in the center of the exposed stub of the ear shoot. This ear shoot is then covered with a small glassine bag and pollinated the next day, when the silks have grown out to a length of about two or three inches and formed a brush of silks. Using such a technique all ear shoots are shortened to about the same length whether the husks were originally long or short. Thus it was possible to develop an inbred whose husk is so long as to make pollination under natural conditions impossible.

We were somewhat surprised the first year we tried to increase C95 in an open pollinated plot to find that almost every ear had no kernels or at best a very few. In 1945 this inbred was increased but the ear shoots in the open pollinated field were cut off just as they are when hand pollinations are made. Figure 13 shows the results of such pruning of the ear shoots.

It is not possible to state whether the sterility of C95 is purely a mechanical function of the long husks preventing the silks getting out in time to be pollinated or whether the silks of 95 are incapable ot waiting a sufficiently long time to become pollinated. In other inbreds it is known that some silks are able to wait a much longer time than others and set seed if they receive pollen. Possibly the silks of C95 remain functional only a comparatively short time.

The inability of C95 to set seed under field conditions makes it a somewhat difficult inbred to increase. It is well adapted for use in a crossing field when C95 is used as the pollinator. Pollen rows set no seed and hence do not draw as heavily on the soil fertility as the seed rows. In some seed-producing areas it is customary to remove the plants in the pollen rows after pollination is completed. By doing this the adjacent seed rows will yield slightly more, since they are able to draw on the extra soil fertility. C95 used as a pollinator automatically eliminates itself as a competitor for soil fertility after its function is completed, thus making an ideal pollen parent.

C95 is a very late yellow inbred sweet corn secured from a cross of P39.C85 × Long Island Beauty after backcrossing once to Long Island Beauty. C95 is used in making late sweet corn hybrids and also in producing sweet dent silage. It is used as the pollen parent. For the seed parent one of several F_1 dent hybrids can be used. The ones used thus far are Hy × Ohio 40B and L317 × 40B. Both of these hybrids when crossed by 95 give a productive stalk and an ear whose grains segregate for three starchy to one sweet. Chemists have found no difference in sugar content of the sweet dent silage and field corn silage. Preliminary tests have shown the cows prefer the sweet-dent silage.

C95 grows to a height of eight to ten feet and is sufficinetly tall to pollinate any of the F_1 dent hybrids commonly used as seed parents. It is a good pollen parent but almost worthless as a seed parent because of the female sterility described here.

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THE "SNOWY BELLY" MOUSE

A Dominant Allele of the Agouti Series in the House Mouse

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Washington, D. C.

FIVE alleles in the agouti series have been reported for the house mouse by various authors. They are described by Keeler.[1] Another dominant allele in the series has been segregated in the progeny of a feral mouse, an adult male, which is the paternal ancestor of the strain described. This male was trapped in a shed in Arlington, Virginia, in May, 1941, and lived until March 25, 1943. Its skin and skull are now deposited in the United States National Museum, Washington, D. C. (No. 271008).

Its color above is ochraceous, suffused with black. The back is very much lighter agouti than that of A^w or A, due to wider yellow banding of the hairs. The whole of the underside is pure white, the hairs being without dark roots. This includes the lower part of the cheeks, also the upper side of the hands to the wrists, and the upper side of the feet to the tarsal joints (hocks). There are a few speckled hairs in the middle line on the wrists and tarsal joints, and in an indistinct ochraceous throat mark. There is a clear ochraceous flank line that separates the sharply contrasted colors of the back and belly. The tail is distinctly bicolored, both the skin and the hairs, the lower side being unpigmented and covered with white hairs, even at the tip. The length of the head and body is 96 mm., that of the tail 90 mm., *i.e.*, the tail is slightly shorter (tail ratio 107).

This color pattern corresponds to that found in the wild strains of *Mus musculus* (*wagneri, spicilegus, spretus*), and to the Mediterranean and Middle Eastern feral mice (*praetextus, bactrianus*),

TABLE I—Data on progeny of the snowy belly male.

Type of mating	Number of progeny of the colors designated						
	Snowy	Light*	A^w	Gray	Black	Other**	Total
F_1 progeny							
Snowy × A^wA^w	9	5	3	–	–	–	17
Snowy × A^w-	2	1	2	1	–	–	6
Snowy × A-	–	–	–	6	–	–	6
Snowy × aa	–	3	–	12	–	–	15
F_2 progeny							
F_1's from A^wA^w	14	24	3	37	–	–	78
F_1's from A^w-	5	6	–	8	–	10	29
Snowy/A^w × Snowy/A	–	2	–	4	–	–	6
F_1's from A-	5	5	–	9	–	1	20
F_1's from aa	–	4	–	11	4	7	26
Backcross progeny							
Snowy × F_1's from A^w-	–	6	–	7	–	–	13
Snowy × F_1's from A-	–	–	–	1	–	–	1
Snowy × F_1's from aa	1	4	–	9	–	–	14
Snowy × F_2's from A^wA^w	9	6	–	14	–	–	29
Snowy × F_2's from A^w-		2	–	–	–	–	2
Snowy × F_2's from aa	1	8	–	6	–	–	15
Snowy × later generations	5	4	–	4	–	–	13
F_3 and later progenies							
F_2 and later generations	456	140	10	152	–	–	758

*These are generally lighter than A^w, not pure white like the male parent, but had a grayish underfur.

**Includes albinos and c^e dilutes. This group was not considered in the F_3 and later generations.

described previously as snowy belly by Schwarz and Schwarz.[2]

This mouse was mated to light-bellied (A^w), gray-bellied (A), and black (aa) females. Data in Table I include the F_1, F_2 and backcross progeny from the original male and the progeny of later generations.

Results suggest that snowy belly is dominant to A^w and to lower alleles in the agouti series. Tests have not been made with A^y, but it probably belongs between A^y and A^w in the series. The column designated light includes those which were not pure white, but generally were lighter than A^w and had a slightly grayish underfur. There is the possibility that some of these were A^w. The original male must have been heterozygous, since he produced both A^w and A, as well as snowy-bellied young. Since this is true, the F_1 progeny should consist of snowy and A^w in the ratio of 1 : 1 from homozygous A^w females, and 2 snowy : 1A^w : 1A from heterozygous A^w females. That these ratios were not obtained is due, undoubtedly, to the small numbers involved and to inability to classify the progeny correctly. This also holds true for the F_2 and backcross progenies.

Selection of breeding stock has been always for the whitest bellies. This has resulted in gradual purification of the strain, so that in the later generations grayish underfur seldom appears. Also crosses of these later generations with lower alleles of the agouti series give a larger percentage of pure white bellies than were found in the earlier crosses.

The term snowy belly is now proposed as a genetic designation for this allele, with A^s as its symbol.

Author's Note: Since this report was submitted for publication, black mice with snowy bellies have appeared in the F_2 progeny from crosses between A^s and a^t. They are not old enough to have been tested to determine their genetic constitution. Probably the simplest explanation is to assume that S is a gene on the same chromosome with agouti, but at a different locus. The snowy-bellied agouti would be AS and other agouti A, with the proper exponent, and s. The snowy-bellied black would represent a cross-over class. It is possible also that a more complex situation may be responsible for the snowy-bellied black.

Literature Cited

1. KEELER, CLYDE E. The Laboratory Mouse. Its Origin, Heredity and Culture. Harvard University Press, Cambridge, Mass. 1931.

2. SCHWARZ, E., and H. K. SCHWARZ. *Jour. Mammal.* 24:(11):59-72. 1943.

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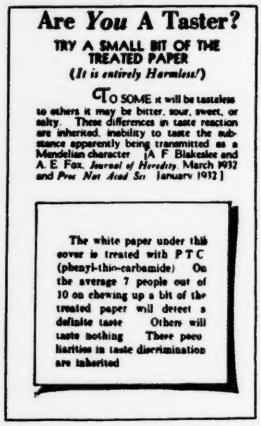
Hollywood Genetics

Too bad to spoil the otherwise beautiful film "Thunderhead," with some impossible genetics. Thunderhead's grandfather, the wild albino, is portrayed as a cremello, which is a true form of albinism. Thus Thunderhead's mother, Flicka, could have inherited (as a hidden recessive trait) from the wild albino either his particular form of albinism, or some form of albinism which is recessive to cremello. But Thunderhead (at least as portrayed in the film) had dark eyes, and presumably a black skin under his white coat, and hence was that kind of "white" horse more properly known as "gray." This coloring is *not* a form of albinism, and hence could not possibly have been a throw-back to the albinism of his grandfather.

I don't know whether it has yet been determined as to whether albinism in horses, as in most rodents, can blank out any and all other color schemes, or on the other hand is merely alternative to the other alleles of the Ysabella series (albino, cremello, huevo de pato, and flaxen-maned chestnut). If the former, then it is possible that a recessive grayness, present in both sire and dam of Thunderhead, and inherited through Flicka from the wild albino, might when coupled with a pigmentation gene (inherited from both parents) account for Thunderhead's gray coloration.

But even so, this would not amount to a throw-back to the grandfather's *albinism*, which is what the story calls for.

Too bad the studios didn't avail themselves of the presence of America's leading geneticist (under whom I studied at Harvard): Dr. William E. Castle of Berkeley. But then, what can you expect in a State where even the Courts (as in the Chaplin paternity case) disregard the established principles of the science of heredity!—RALPH MILNE FARLEY, *National Horseman*, Nov. 1945.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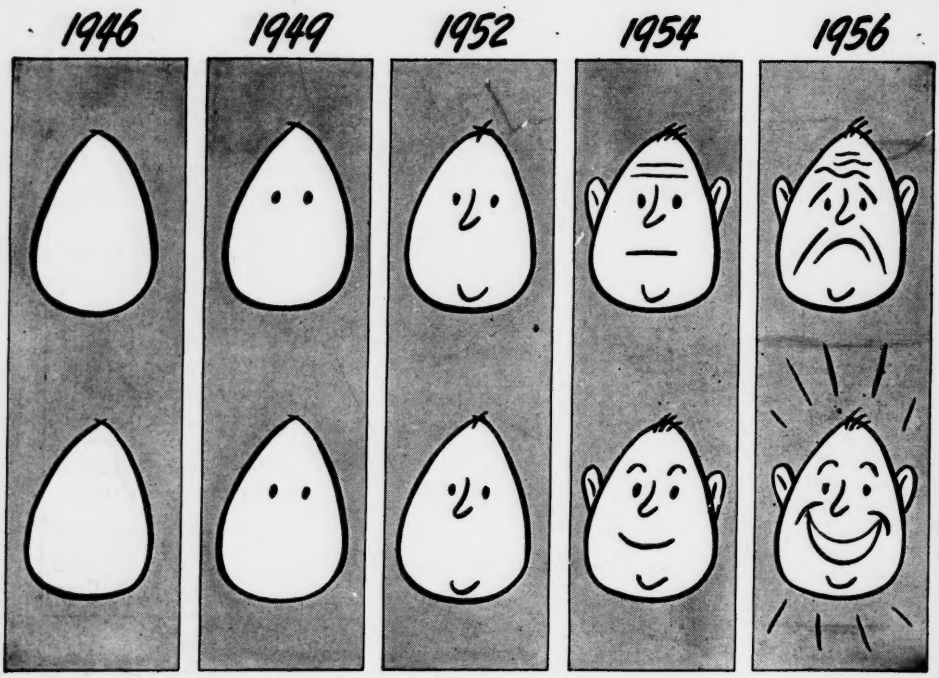

Two ways your face can grow in the next few years

USUALLY, our faces show what's happening to us.

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